

Comment on "Poynting vector, heating rate, and stored energy in structured materials: A first principles derivation"

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In this Comment I argue that Silveirinha's criticism of my earlier work [M.G.Silveirinha, *Phys. Rev. B*, **80**, 235120 (2009)] is based on an unphysical excitation model which involves an "external current" which overlaps with a continuous medium but is not subject to constitutive relations. When this excitation model is replaced by the conventional and experimentally-relevant model of excitation by external electromagnetic fields, it can be easily shown that Silveirinha's formulas contain the very results he wanted to disprove. This and a few other misconceptions present in Silveirinha's paper are subject of this Comment.

I. INTRODUCTION

This paper contains a comment on the paper¹ by M.G. Silveirinha entitled "Poynting vector, heating rate, and stored energy in structured materials: A first-principles derivation" [*Phys. Rev. B* **80**, 235120 (2009)].

As is customary for all APS journals, the Comment was first sent to Silveirinha for review. Silveirinha has written an 11-page report on my Comment and recommended its rejection. I disagreed, the Comment went through the ordinary process of peer-review and was eventually rejected. Still, I did not agree with the scientific arguments used to justify the rejection. I have then reworked the Comment into a stand-alone paper with independent conclusions. This paper² is now published and is entitled "On the current-driven model in the classical electrodynamics of continuous media" [*J. Phys.-Cond. Matt.* **22**, 485401 (2010)]. Although the above publication makes all the essential points, I still feel that the original Comment should be made available to the community as it is, in some respects, more direct and specific.

Finally, it should be mentioned that a different comment³ on the same paper¹ by Silveirinha has been submitted and published.

II. THE ORIGINAL COMMENT

Recently, Silveirinha has criticized¹ several earlier papers⁴⁻⁶, two of which are authored by myself. In particular, Silveirinha has stated that the conclusions made by me in Ref. 4, as well as similar conclusions made by Richter *et al.* in Ref. 5, "are founded on fundamental misconceptions and mistakes". To support this claim, Silveirinha has offered a rather lengthy derivation which, according to Silveirinha, disproves the results of Refs. 4 and 5. However, I have found that Silveirinha's derivation itself is based on a few misconceptions. The chief among these is the contention that an electromagnetic wave with a purely real wave vector can propagate in absorbing materials (either "meta-" or otherwise), that such non-decaying waves can be obtained by imposing an

"external current" which overlaps with the medium but is not subject to constitutive relations, and that solutions thus obtained can be used to derive information about the rate at which the medium is heated by radiation (the heating rate). This and a few other "misconceptions and mistakes" which are present in Silveirinha's work are subject of this Comment. Gaussian system of units is used throughout. Equations of Silveirinha's paper are referenced as (S#).

A) The assumption of purely real wave vector and the external current. Most of Silveirinha's derivations, including all derivations that lead to results of any practical significance, are carried out for Bloch waves with a purely real wave vectors \mathbf{k} . Silveirinha is, of course, aware that natural Bloch modes in periodic media with some amount of absorption are, necessarily, decaying waves. In a typical experiment, these decaying modes are excited by external radiation. The decay is mathematically represented by the imaginary part of \mathbf{k} . Thus, if \mathbf{k} is taken to be purely real, the medium is, by definition, non-absorbing. Calculation of the heating rate in such a medium is meaningless: under the condition $\text{Im} \mathbf{k} = 0$, any reasonable calculation must yield zero.

Silveirinha, however, contends in his paper that a real-valued Bloch wave vector \mathbf{k} is not incompatible with losses. He argues that one can consider an *external current* rather than an external radiation as the source for the electromagnetic wave in the medium. This is quite fine as long as the current and the medium do not overlap. But Silveirinha requires that the external current be of the form $\mathbf{j}_e = \mathbf{A} \exp(i\mathbf{k} \cdot \mathbf{r})$, where \mathbf{A} is a constant amplitude, so that \mathbf{j}_e is nonzero in the region of space occupied by the medium. It must be emphasized that the current \mathbf{j}_e introduced by Silveirinha is not created by the charges associated with the medium, does not obey any constitutive relations (and, therefore, is independent of the fields existing in the medium), and, presumably, can be manipulated by the experimenter at will.

The excitation model proposed by Silveirinha and described briefly above is unphysical because it can not be realized experimentally. The obvious question Silveirinha

should have answered is the following: how can an external current of the form $\mathbf{j}_e = \mathbf{A} \exp(i\mathbf{k} \cdot \mathbf{r})$ be physically created in the medium? If we set aside heating, mechanical stress, acoustic waves and other similar influences (which, anyway, are highly unlikely to generate the required current), we are left with the possibility of applying external electromagnetic fields to the medium. But any current produced in such an experiment is subject to the usual constitutive relations and is, therefore, a part of the induced current which is denoted in Ref. 1 by \mathbf{j}_d . Another option is to insert multiple wires into the medium and to run externally-controlled currents through the wires. This will, of course, destroy the medium and no reasonable experimentalist would propose or try to implement such an excitation scheme. It is also highly unlikely that pre-determined spatially-varying currents can be maintained in the wires by external voltages.

Thus, the idea of exciting the medium with a pre-determined external current which overlaps with the medium is unphysical and can not be used to derive any useful physical quantity. But even from a purely formal point of view, any results derived from such an excitation model can not be used to criticize Ref. 4 because in this paper I have clearly and repeatedly stated that I consider only the case when all currents in the medium are induced and there are no external or “free” currents within the medium. (Note that the conductivity current is included in the induced current in this paper and in Ref. 4.)

B) Heating rate with and without the assumption of a purely real wave vector. Now let us consider how Silveirinha computes the heating rate. The starting point will be Eq. (S60). To save a few notations, I will re-write this equation in Gaussian units and omit all subscripts. Then Eq. (S60) takes the form

$$q = \frac{\omega}{8\pi} \text{Im} \left[\mathbf{E}^* \cdot \hat{\epsilon} \mathbf{E} + \left(\frac{c}{\omega} \right)^2 \mathbf{E}^* \cdot \mathbf{k} \times (\hat{\mu}^{-1} - \hat{I}) \mathbf{k} \times \mathbf{E} \right]. \quad (1)$$

Here tensors (dyadics) are denoted by a hat, the symbol “.” is reserved for a dot product of two vectors, no special symbol is used to denote the action of a tensor on a vector (for example, $\hat{\epsilon} \mathbf{E}$), and all operations are evaluated from right to left; \mathbf{E} is the macroscopic electric field and \hat{I} is the unit tensor. All fields are assumed to be monochromatic and the common exponential factor $\exp(-i\omega t)$ is suppressed. Note that I have used the first identity in (S36) to transform (S60) to the form (1). In fact, (1) is equivalent to (S60) but is written using more conventional tensor notations.

Silveirinha evaluates expression (1) as follows. He uses the vector identity $\mathbf{a} \cdot (\mathbf{b} \times \mathbf{c}) = (\mathbf{a} \times \mathbf{b}) \cdot \mathbf{c}$ (equivalent to the second identity in (S36)) to re-write the second term in the square brackets as

$$\left(\frac{c}{\omega} \right)^2 (\mathbf{E}^* \times \mathbf{k}) \cdot [(\hat{\mu}^{-1} - \hat{I}) \mathbf{k} \times \mathbf{E}]. \quad (2)$$

Then Silveirinha uses Eq. (S10a), namely, $\mathbf{k} \times \mathbf{E} = (\omega/c) \mathbf{B}$. This equality follows from the macroscopic Maxwell’s equations applied to a plane wave. It can be seen that the factor $\mathbf{k} \times \mathbf{E}$ in the end of expression (2) can be replaced by $(\omega/c) \mathbf{B}$. However, the expression $\mathbf{E}^* \times \mathbf{k}$ in the beginning of this expression can only be replaced by $-(\omega/c) \mathbf{B}^*$ if \mathbf{k} is purely real. Silveirinha makes this assumption about \mathbf{k} and transforms (2) to the form

$$- \mathbf{B}^* \cdot (\hat{\mu}^{-1} - \hat{I}) \mathbf{B} \quad (3)$$

The term proportional to \hat{I} is then omitted since its imaginary part evaluates to zero, the field \mathbf{B} is expressed in terms of the field \mathbf{H} using the constitutive relation $\mathbf{B} = \hat{\mu} \mathbf{H}$ and Silveirinha arrives at his Eqs. (S61) and (S62). The important point here is that the transition from (S60) to (S61) and (S62) requires that $\text{Im} \mathbf{k} = 0$.

But if we set aside the unphysical model in which the medium is excited by an external current which overlaps with the medium, then \mathbf{k} is not a mathematically independent variable but is determined from the Maxwell’s equations and constitutive relations. The equality $\text{Im} \mathbf{k} = 0$ is only possible if $\text{Im} \hat{\epsilon} = \text{Im} \hat{\mu} = 0$. Therefore, the final result for the heating rate derived by Silveirinha, Eq. (S62), evaluates to zero under the assumption that was used to derive it. It can be concluded that the result (S62) is not a “first-principle, completely general” derivation of some useful physical quantity, as Silveirinha has claimed, but a lengthy and laborious proof of the identity $0 = 0$.

It is possible, however, to evaluate the expression (1) differently without making any assumptions about \mathbf{k} . To this end, we use the two Maxwell’s equations

$$\mathbf{k} \times \mathbf{E} = \frac{\omega}{c} \hat{\mu} \mathbf{H}, \quad \mathbf{k} \times \mathbf{H} = -\frac{\omega}{c} \hat{\epsilon} \mathbf{E}. \quad (4)$$

Here I have assumed that all currents in the medium are induced and subject to constitutive relations. From (4), we can also obtain

$$\mathbf{k} \times \hat{\mu}^{-1} \mathbf{k} \times \mathbf{E} = - \left(\frac{\omega}{c} \right)^2 \hat{\epsilon} \mathbf{E}. \quad (5)$$

Substitute this expression into (1). The term proportional to $\hat{\epsilon}$ will cancel to yield

$$q = -\frac{\omega}{8\pi} \left(\frac{c}{\omega} \right)^2 \text{Im} (\mathbf{E}^* \cdot \mathbf{k} \times \mathbf{k} \times \mathbf{E}). \quad (6)$$

Next, use the identity $\mathbf{k} \times \mathbf{k} \times \mathbf{E} = \mathbf{k}(\mathbf{k} \cdot \mathbf{E}) - k^2 \mathbf{E}$ to obtain

$$q = \frac{\omega}{8\pi} \left(\frac{c}{\omega} \right)^2 \text{Im} [|\mathbf{E}|^2 k^2 - (\mathbf{E} \cdot \mathbf{k})(\mathbf{E}^* \cdot \mathbf{k})]. \quad (7)$$

This expression is equivalent to the one derived by me earlier in Ref. 4 (Eq. (54) of this reference) for the case

of general anisotropic nonlocal media. If the medium is isotropic, it can support only transverse waves whose wave number is $k^2 = (\omega/c)^2 \varepsilon \mu$ with ε and μ being scalars. Then (7) is simplified to

$$q = \frac{\omega |\mathbf{E}|^2}{8\pi} \text{Im}(\varepsilon \mu) . \quad (8)$$

Again, this result was derived by me in Ref. 4 (see Eq. (32)).

Thus, it can be seen that Silveirinha's Eq. (S60) contains, in fact, the very results he wanted to disprove. The only reason Silveirinha has obtained a formula which appears to be different from (7) or (8) is because he has used a method to evaluate his equation (S60) which is only valid when $\text{Im} \mathbf{k} = 0$. My method of evaluating (S60) makes no assumptions about \mathbf{k} . It is applicable, in particular, when $\text{Im} \mathbf{k} = 0$. In this case, (7) and (8), as well as Silveirinha's results (S61), (S62), all evaluate to zero and are, in this sense, equivalent. But, unlike Silveirinha's results, formulas (7) and (8) can also be used in the physically interesting case of a complex wave vector \mathbf{k} .

Silveirinha would, perhaps, argue that, for complex wave vectors, (S60) is itself invalid because its derivation requires that the second term in the right-hand side of (S58) be neglected. This term was shown to be exactly zero when $\text{Im} \mathbf{k} = 0$. However, in electromagnetically homogeneous media, this term is negligibly small anyway. This is evident from the following consideration. The tensor \hat{G}_{p0} defined in (S57) is real and Hermitian as long as $\mathbf{k} \cdot (\mathbf{r} - \mathbf{r}') \ll 1$. Since the double integration in (S58) is over the volume of the same elementary cell, the maximum phase shift $\mathbf{k} \cdot (\mathbf{r} - \mathbf{r}')$ is of the order of or smaller than $|\mathbf{k}|h$, where h is the characteristic cell size. For the medium to be considered electromagnetically homogeneous, this phase shift must be negligibly small. Therefore, the result of double integration in (S58) is purely real with the same precision as the precision of the underlying approximation. In other words, the errors that a made when the original periodic medium is replaced by a spatially-uniform medium with effective parameters are of the same order of magnitude as the errors which are made when the last term in (S58) is neglected.

C) The lack of equivalence between magnetization and nonlocality. Another serious misconception perpetrated by Silveirinha is that, at high frequencies, magnetic response of matter can be understood and is physically indistinguishable from nonlocality in the dielectric response. To be fair, we should note that Silveirinha did not come up with this misconception on his own. The misconception can be traced to the highly respected book by Landau and Lifshitz⁷ and was recently popularized by Agranovich⁸. However, it is elementary to see that no such equivalence exists.

Indeed, consider the induced current in a medium with electric and magnetic polarization. Let, for simplicity, the medium be isotropic with scalar coefficients ε and μ .

For monochromatic fields, the induced current is

$$\mathbf{J} = -i\omega \mathbf{P} + c \nabla \times \mathbf{M} , \quad (9)$$

where $\mathbf{P} = \chi_e \mathbf{E}$ and $\mathbf{M} = \chi_m \mathbf{B}$, $\chi_e = (\varepsilon - 1)/4\pi$ and $\chi_m = (\mu - 1)/4\pi\mu$. Traditionally (e.g., in Landau and Lifshitz's book or in Agranovich's review), the expression (9) is evaluated for a plane wave with the wave vector \mathbf{k} by writing

$$\nabla \times \mathbf{M} = \nabla \times \chi_m \mathbf{B} = \chi_m \nabla \times \mathbf{B} = i \frac{c}{\omega} \chi_m \mathbf{k} \times \mathbf{k} \times \mathbf{E} . \quad (10)$$

Thus, it is found that the current can be equivalently written as

$$\mathbf{J} = -i\omega \frac{\hat{\varepsilon}_{\text{nonl}}(\omega, \mathbf{k}) - \hat{I}}{4\pi} \mathbf{E} , \quad (11)$$

where

$$\hat{\varepsilon}_{\text{nonl}}(\omega, \mathbf{k}) = \varepsilon \hat{I} - \left(\frac{c}{\omega} \right)^2 \frac{\mu - 1}{\mu} \mathbf{k} \times \mathbf{k} \times . \quad (12)$$

Upon observing that exactly the same induced current is obtained in a medium with permeability equal to unity and nonlocal permittivity $\hat{\varepsilon}_{\text{nonl}}(\omega, \mathbf{k})$ as the current obtained in a medium with local permittivity $\varepsilon \neq 1$ and local permeability $\mu \neq 1$, the conclusion is drawn about the physical indistinguishability of the effects of magnetization and nonlocality of the dielectric response.

The above line of reasoning is erroneous because $\nabla \times \chi_m \mathbf{B} \neq \chi_m \nabla \times \mathbf{B}$. The correct formula is

$$\nabla \times \chi_m \mathbf{B} = \chi_m \nabla \times \mathbf{B} + (\nabla \chi_m) \times \mathbf{B} . \quad (13)$$

The last term in the above equation is often lost sight of because it is assumed that $\chi_m = \text{const}$ and, consequently, $\nabla \chi_m = 0$. However, even if the medium is quite uniform spatially, it always has a boundary, and at the boundary the function $\chi_m(\mathbf{r})$ experiences a jump. Differentiation of this jump gives rise to a surface current which can not be accounted for in the model of nonlocal permittivity (12). To be more precise, accounting for the surface current within the spatial nonlocality model would require the use of position-dependent and highly singular integral kernels in the influence function which would contain derivatives of the delta function. This possibility is never considered. In particular, in Refs. 7,8, the surface current is simply ignored.

Thus, the deficiency of the equivalence model is that it places the emphasis on the dispersion relation but ignores the medium boundary. Although infinite boundless media can be considered as purely mathematical abstractions, such media do not exist in nature. And a dispersion relation alone does not characterize a finite-size sample: the latter also has an impedance. Indeed,

the dispersion relation which follows from (11),(12) is the familiar equation $k^2 = (\omega/c)^2 \varepsilon \mu$, where I have used the fact that waves in isotropic media are transverse. This dispersion relation is invariant with respect to the transformation $\varepsilon \rightarrow \beta \varepsilon$, $\mu \rightarrow \beta^{-1} \mu$, where β is an arbitrary complex constant. In particular, we can take $\beta = \mu$. It then would appear that a medium with some values of ε and μ is indistinguishable from a medium with $\varepsilon' = \varepsilon \mu$ and $\mu' = 1$. But the two media have different impedances and, therefore, different transmission and reflection properties.

D) The meaning of homogenization. Yet another misconception encountered in Silveirinha's work is related to the notion of homogenization. Silveirinha seems to believe that homogenization is equivalent to field averaging. However, fields can always be averaged but not every medium is electromagnetically homogeneous at a given frequency. The goals of homogenization are the following. Firstly, one needs to prove that solutions to the macroscopic Maxwell's equations in a nonmagnetic spatially-inhomogeneous medium characterized by the "true" permittivity $\varepsilon_{\text{true}}(\mathbf{r})$ converge in some physically reasonable norm to solutions in a medium of the same overall shape but with spatially-uniform effective parameters ε_{eff} and μ_{eff} . Secondly, the effective parameters must be computed explicitly in terms of $\varepsilon_{\text{true}}(\mathbf{r})$. The original and the effective media must be "electromagnetically similar" for varying incident fields and varying shapes of the sample.

None of this has been done in Ref. 1. Silveirinha did not consider the conditions under which spatial averaging of fields is physically meaningful, nor did he derive the effective parameters in terms of $\varepsilon_{\text{true}}(\mathbf{r})$. True, more was done by Silveirinha in Ref. 9, where a numerical procedure to determine the effective parameters was proposed and implemented. However, this development was not used in Ref. 1. Besides, Ref. 9 is affected by the same unphysical excitation model as Ref. 1: the Bloch wave number \mathbf{k} is viewed in both references as a mathematically-independent variable "imposed" by the external current although it is well known that to solve a photonic crystal, \mathbf{k} must be computed self-consistently. In any case, Silveirinha's derivation presented in Ref. 1 is simply a chain of definitions which, after some terms are neglected, result in familiar macroscopic formulas, such as (S60) (which Silveirinha then evaluates incorrectly). These formulas contain effective parameters which, at that point, are purely phenomenological and which have not been computed either "from first principles" or in any other meaningful way. For example, in Eq. (S60), there appear some effective medium parameters ε_r and μ_r . How are these related to $\varepsilon_{\text{true}}(\mathbf{r})$? How does Silveirinha know that μ_r is different from unity?

In this respect, an interesting result has been recently reported by Menzel *et al.*¹⁰ It was found that a typical "metamaterial" can not be reasonably characterized by effective parameters in the spectral region in which magnetic resonances are excited. This result indicates that,

contrary to Silveirinha's assertion, "artificial magnetism" can not be obtained in electromagnetically homogeneous materials. Here, following Ref. 11 and a naive analogy with natural magnetism, Silveirinha has confused magnetic polarizability of subwavelength particles with magnetic permeability of a periodic medium made of such particles. In the case of natural magnetism, it is true that magnetically polarizable atoms can assemble to form a medium with nontrivial permeability. However, natural magnetism is a quantum effect which does not disappear at zero frequency. The so-called artificial magnetism is a conceptually different and a purely classical phenomenon. In particular, it should be noted that while the magnetic polarizability of a particle is always defined, and can, indeed, be large if Ohmic losses are sufficiently small, the effective permeability of a medium is not always defined. The results of Ref. 10 indicate that the effective parameters can only be introduced far away from magnetic resonances.

Silveirinha's correctly writes that the quadratic fluctuations of the field of the type $\langle \delta \mathbf{e} \times \delta \mathbf{b} \rangle$ (\mathbf{e} and \mathbf{b} are the "microscopic" electric and magnetic fields) can not be neglected in the *general case*. However, in Ref. 4, I was not concerned with the general case but only with the case of electromagnetically homogeneous media which can be reasonably characterized by effective medium parameters. In such media, the quadratic fluctuations can be and should be, in fact, neglected. Accounting for such fluctuations amounts to exceeding the precision of the underlying approximation (that is, the approximation in which the medium is characterized by spatially-uniform effective parameters). Silveirinha did not demonstrate that the quadratic fluctuations can be large while the medium is still electromagnetically homogeneous according to the criteria stated above. And the quantitative arguments given in this Comment are quite indicative of the contrary.

E) Unsubstantiated criticism. Silveirinha's criticism of Ref. 6 which appears after Eq. (S62) is not based on any scientific arguments. To be more specific, Silveirinha has derived (S62) and stated, essentially, that it follows from (S62) that Ref. 6 is erroneous as shown by Efros. However, the argument of Ref. 6 is based on the traditional expression for the heating rate (the one equivalent to (S62)) and the alternative expression is only mentioned as a possibility. Also, the arguments of the *earlier* paper by Efros¹² are considered and discussed in Ref. 6 at length. Finally, the main subject of Ref. 6 are natural diamagnetics. It almost appears that Silveirinha did not look at the content of Ref. 6 at all.

In summary, I have shown that the main conclusions of Silveirinha's paper are based on an unphysical excitation model in which an external current which overlaps with the medium but is not subject to constitutive relations is used. However, if we use instead the conventional and experimentally-relevant model of excitation by external fields, we would find that Silveirinha's formulas contain the very results he wanted to disprove. I have

demonstrated this explicitly for the heating rate, q . I did not consider the density of electromagnetic energy as this quantity is not measurable experimentally and is, therefore, irrelevant (only the total electromagnetic energy of a body is measurable, but this quantity is the same in the conventional theory and in the theory of Ref. 4). As for the Poynting vector, \mathbf{S} , in a steady state and, particularly, for monochromatic fields, it must satisfy the continuity equation $q + \nabla \cdot \mathbf{S} = 0$ (time averaging is assumed). I have shown that the quantity q that follows from Sil-

veirinha's Eq. (S60) is exactly the same as I have derived in Ref. 4. I have also shown in Ref. 4 that the expression for \mathbf{S} which contains the cross product $\mathbf{E} \times \mathbf{H}$ is not consistent with this expression for q while the expression which contains $\mathbf{E} \times \mathbf{B}$ is. Therefore, if Silveirinha adopts the correct excitation model and uses it to re-compute the Poynting vector, he would surely find an expression which is quite in agreement with my previous results and with Silveirinha's own Eq. (S60).

¹ M. G. Silveirinha, Phys. Rev. B **80**, 235120 (2009).

² V. A. Markel, J. Phys.: Condens. Matter **22**, 485401 (2010).

³ F. Richter, K. Henneberger, and M. Florian, Phys. Rev. B **82**, 037103 (2010).

⁴ V. A. Markel, Opt. Express **16**, 19152 (2008).

⁵ F. Richter, M. Florian, and K. Henneberger, Europhys. Lett. **81**, 67005 (2008).

⁶ V. A. Markel, Phys. Rev. E **78**, 026608 (2008).

⁷ L. D. Landau and L. P. Lifshitz, *Electrodynamics of Continuous Media* (Pergamon Press, Oxford, 1984).

⁸ V. M. Agranovich and Y. N. Gartstein, Physics-Uspekhi **49**, 1029 (2006).

⁹ M. G. Silveirinha, Phys. Rev. B **75**, 115104 (2007).

¹⁰ C. Menzel, T. Paul, C. Rockstuhl, T. Pertsch, S. Tretyakov, and F. Lederer, Phys. Rev. B **81**, 035320 (2010).

¹¹ J. B. Pendry, A. J. Holden, D. J. Robbins, and W. J. Stewart, IEEE Trans. Microwave Theory and Techniques **47**, 2075 (1999).

¹² A. L. Efros, Phys. Rev. E **70**, 048602 (2004).